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# To the Editor

## 'The Ultra Secret'

Sir—All historians will note with interest F. W. Winterbottom's further elucidation in your column of June 25, of his role in the Ultra secret, and his denial that he was responsible for any of Cava Brown's remarks or paragraphs on this subject such as Churchill having forty-eight hours notice and sacrificing the city [of Coventry] for the sake of Ultra.

In his book *The Ultra Secret* (pages 50-51) Group Captain Winterbottom certainly refutes the legend that we had 48 hours' notice of the raid. But he goes on to describe how, at about 3 p.m. on November 14, he informed Churchill through his private secretary that Coventry had been identified as the target for that night; how he waited expectantly because Churchill decided to evacuate Coventry that night, and indeed, everybody would know by the knowledge of the raid and some counter-measures might be necessary to protect the source which would obviously become suspect. . . . In the event it was decided only to allow a few minutes' notice, the raid was cancelled. . . . This is the sort of terrible decision that sometimes has to be made on the highest levels in war. It was unquestionably the right one (my italics).

Here, surely, we have an unmistakable suggestion that measures which might have been taken for the defence of Coventry were deliberately foregone in order to protect the Ultra secret; and it was presumably on the strength of this paragraph that Cava Brown erected his highly commendable and imaginative account. One sympathises with Group Captain Winterbottom, but he cannot so easily escape responsibility for Cava Brown's spectacular allegations.

MICHAEL HOWARD,  
All Souls College, Oxford.

Sir—One of the main purposes of F. W. Winterbottom's letter in your issue of June 25, is plainly to draw a distinction between, on the one hand, Michael Howard and me, whom he describes as "eminent historians," and on the other hand himself, whom he represents as the practical man who "was there." I am flattered both by the company in which I find myself and by the description, which is certainly no more than Michael Howard deserves, but to give more weight to what follows let me assure your readers that I was there too in the sense that F. W. Winterbottom intended. From 1941 to the end of the war I regularly read Ultra signals, and took action on the basis of them, so that I have practical knowledge of their use in operations. In consequence I can say with confidence that it is an officer's duty of intelligence during the war to publish the value of the books about which Michael Howard and I were writing.

On Coventry Mr Winterbottom repeats the error of his book. Let me set out the case clearly. He sets out two propositions which I deny: (a) that the designation of Coventry as one of the targets for the operation Moonlight Sonata was given in an Ultra signal; (b) that a decision taken by Churchill, against the advice of the Ultra secret, was responsible for the raid.

When I called Mr Winterbottom's *The Ultra Secret* "generally unreliable, especially in accounts of operations," I was not relying on my own knowledge of the Ultra secret, but on my knowledge of the campaign he describes, particularly those in Africa, Sicily and Italy, during which I was both fully in the operational picture and a regular recipient of all the Ultra signals. Mr Winterbottom on the other hand, was principally concerned with making the administrative arrangements for the distribution of the Ultra material. In consequence I find his judgment faulty not only on such matters as those to which he drew attention in my review of December 18—his mis-

lead the city for the sake of preserving the Ultra secret.

On (a) he writes in his book "at about 3 p.m. on November 14th someone must have made a slip up and instead of a city with a codename [sic] Coventry was spelt out." In his letter he refers to "the signal I sent over on November 14, giving the target for that night as 'Coventry' in clear." This is incorrect: from evidence now in the PRO, supplemented by persons who know what Ultra messages were received at that time it is certain that the name was not given, either in clear or in cipher, of that the identification of Coventry as the target came from plotting the intercepts of the X-Gerät beams.

The falsity of (b) I demonstrated in my review (May 28), proving from witnesses who were on the spot at the time, and in a uniquely authoritative position to know, that Churchill had no idea when the blow would fall. My principal witness, Sir John Colville, really is an eminent historian, but "was there" and he kept a diary.

I must make in passing a correction to my review: it was Sir John Colville, not Sir John Peck, who was in the car with Churchill; Peck and Colville were both on No 10 that evening when the car returned, Churchill having ordered it to turn round before he left London. Martin Colville confirms the account on which I based myself. I notice that in his letter Mr Winterbottom does not repeat the statement he makes in his book that he "spoke directly to Churchill's personal secretary" in doing so he is in line with my inquiries of the people then on No 10 who have no recollection of such a telephone conversation.

I am not sure of the intention of the contrast Mr Winterbottom is apparently trying to make in two sentences in the middle of his letter: "On the evening of 10 November 14 I went to my cottage, west of London and counted the enemy bombers going overhead on their deadly mission. I feel sure that Sir David [sic] there, he too might have felt a little 'ogony' for the people of Coventry." Yes, indeed; and for the people of London and Birmingham, who, as part of the same operation, suffered equally heavy attacks on the nights of the 13th and 14th respectively without any allegations of their having been sacrificed. But the "ogony" I referred to in my review, picking up the expression he used in his book, was not over the bombing but over the supposed difficulty of the alleged decision to leave them to their fate; and I was concerned to demonstrate that there was no such decision.

On the question of the origin of the code name "Ultra" Mr Winterbottom in his review directed attention to a sentence in his book, "William Stevenson who claims to have invented the designation in 1940 along with a Signal, Petroleum Warfare and the Prime Minister used yet another designation which cloaked the origin of the information by attributing it to a supposed 'agent' called 'Bontemps'." (p. 118). In 1944, the name "Ultra" became more general and it is now, for reasons of convenience, the standard term used by historians.

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less story of a secret visit to Italy by Churchill in May 1944 and his belief that the Overlord plans "had been stolen by a German spy from the British Ambassador in Ankara"—but also on military operational matters as a whole. For the sake of fairness I will take an example from outside my personal experience. Mr Winterbottom writes "at the end of May 1944 on 'Ultra' signals . . . reported that fifty IVI signs were ready for launching. This finally determined Churchill to press for the start of Overlord in June at all costs." He seems to be unaware that June 5 had been picked as the start of Overlord long before, that by the end of May preparatory moves had already begun and that for Churchill to "press" for an earlier date was neither necessary nor possible.

It is the evidence of such simple examples of a fundamental incomprehension of the nature of military operations, rather than of detailed errors on matters of detail, that convinces me that Mr Winterbottom's book will never be used as a primary source by serious historians.

SIR DAVID HUNT,  
Old Place, Lindfield, Sussex RH16 2HU.

## An Eighteenth-Century STC

Sir—Students of eighteenth-century England, whatever their persuasion, must welcome Nicolas Barker's report (July 2) on the recent eighteenth-century STC Conference at the British Library. Eighteenth-century scholars have glanced back enviously at the STC working in earlier periods. The one for an eighteenth-century scholar seems favourable: a number of "spoke" direct to Churchill's personal secretary" in doing so he is in line with my inquiries of the people then on No 10 who have no recollection of such a telephone conversation.

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## Velikovsky

Sir—In the issue of June 25 your reviewer, John North, devoted some three half thousand words to discussion of Dr Immanuel Velikovsky's controversial theories and the recent book *Velikovsky Reconsidered* by the editors of *Pensée*. In the course of the review the Society for Interdisciplinary Studies was mentioned. I hope that I may be allowed a few hundred words in comment. This society, while far from believing Velikovsky infallible, considers that he deserves a better hearing than he has so far been given. Over the years of controversy this view has been supported by such eminent men in their fields as Walter Orr Roberts, past president of the American Association for the Advancement of Science; Professor H. H. Moseley, late chairman of the University of Toronto; and Bruno de Finetti, professor of mathematics at the College de France; and Jacques Barzun and Alfred Einstein.

The political side of the controversy has been of interest to this society, our prime object being to consider the status of the evidence in the various fields and to this end I would like to take issue with some of Dr North's more misleading statements. In his references to the Venus tablets of Amarna, Dr North avoids the central issue and is Professor Rose's paper which is that the data on the tablets can only be made to correlate with the present notions of Venus by assuming an arbitrary degree of scribal error, yet it takes as an accurate record of the ancient observations the orbits shown can be reconciled with Velikovsky's thesis. At this point Dr North invokes Laplace, and this disposes of Velikovsky's case: "If he [Laplace] had had reason for supposing that Jupiter was capable of exploding, he would have added a caveat. . . . In fact Laplace added several caveats to his formulae, specifically that they did not allow for perturbations due to comets or other unpredictable forces; such as meteorites striking the earth. Dr North is in any case out of touch with the relevant literature; Professor R. W. Bess, a specialist with impeccable academic credentials, has published several papers (two of them in *Pensée* IVR 8) in which he surveys the current standing of the current literature on the stability of the solar system as shown by the fallacious in 1899 by Poincaré. In 1953 dynamical astronomer W. M. Smart proved that the maximum interval of reliability of Laplace's calculations was in the order of 10<sup>6</sup> years. Bess demonstrates in detail that it is perfectly possible, according to Newton's laws of dynamics and gravitation when three or more bodies are involved, to planets to nearly collide and then relax into an apparently stable orbit. Bess's Law type of configuration is not a relatively short time interval. It is not a relatively short time interval. It is not a relatively short time interval.

Obviously the decision to include or exclude some of the more difficult material must be based to some extent on bibliographical precision, but an equally valid consideration is whether the material included will be useful for future development. In the eighteenth-century studies, Barker is right there is a trap by genre. To illustrate from Dr Speck's own field, it might seem that the literature of parliamentary elections should be omitted, because much of it is difficult to defuse bibliographically. There is, however, much electoral material which has been recorded if an STC is to be of real research use. This is one belongs to the bibliographer rather than to the historian.

Would I receive support for the suggestion that the outstanding continuing as soon as possible a broadly based users' advisory group? CLYDE JONES, Institute of Historical Research, Senate House, London WC1E 7HU.

A Freudian Slip  
Sir—I wonder why Janet Adam Smith thinks it was a Freudian slip of Michael Roberts to type 5 instead of 5 (June 18). It seems much more likely to have been a useful use of the slip-key, particularly as many typists are less at ease when using the top line of keys. N. CHAPLIN, 24 Leathbridge Gardens, London W11 2PY.

of an inert gas" presumed to be mainly argon, in the *Worlds in Collision* of the prehistoric scientific reaction appears sceptical of his theory of violence. The documentation included in *This Velikovsky* (edited by A. de Grazia, Stanford and Jackson, 1966) is further allowed readers to make up their own mind on that score. Dr North's handling is not so good as this cannot compensate for the denial of Velikovsky's theory which denounces his theories even in personal letters.

It is a pity that the review by Slidgwick and Jackson in the next volume of the *Age of the Series, Peoples of the Sea*, months' time, the reviewers themselves to a distance of the evidence presented, in past twenty-six years, despite an unfavourable atmosphere around Dr Velikovsky's ideas, still support for them has continued. It is now time that the prejudices were discarded and theories submitted to the test of study which they deserve as grounds of their success in predicting otherwise unpredictable events. In the words of the editor Melnick of Toronto University: "It does not matter whether Velikovsky's ideas are correct or not, but it is important that the ideas stimulate thought and experiment, and move us to a wider and deeper understanding of nature."

Society for Interdisciplinary Studies, 18 Fir Tree Court, All Lane, Elstree, Hertfordshire. Sir—May I commend on a point to John North's article in *Velikovsky* (June 25)? 1. Alleged duplication of evidence. It is essential to be aware that when one repeats uncritically a story in another, the number of sources is not thereby increased. Dr North includes in this charge Velikovsky's references to Herodotus, Strabo, Pliny, Ptolemy, and other ancient writers. This charge is unfounded. Velikovsky is quoted in *Worlds in Collision* (pages 91-92) because he had access to ancient manuscripts which no longer exist (Velikovsky quotes that Rockingham House and Camperdown and Potosir; Herodotus because he quotes authors like Herodotus, Herodotus, and so on. This is clearly a mistake. 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Ideologically, however, there was much to be gained by this approach. It could serve to reviv-

the short run path on which the economy is moving becomes evident. It will determine the outcome of the process of adjustment.

often irrationally decides on the actual present path from among a number of potential paths. This bedevils the dominant

what I felt was the subtle trans-  
motion of, or identity ex-post, in  
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fish speculative expectations, or alter excessive pessimism), which are the unavoidable concomitants of the boom (or the slump).

...limits of its domestic  
consequences. Hence a pattern  
...came to characterise  
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appeared elsewhere, The Economist of the Oil Crisis is not exactly best buy from amongst the numerous books now available on the

the levels—and although all the contributions were originally written to three years ago (and are

particularly inflation—and  
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ways, however, the odds dim

"off-crisis," to which the economic system will not adjust—almost automatically.

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## Troubles in the pipeline

T. M. BYRCZYNSKI (Editor)

**The Economics of the Oil Crisis**  
202pp. Macmillan, for the  
Policy Research Centre, £10.

This book is the first volume of papers to be published in the Trade Policy Research Committee programme of studies on reform of the international trade system. Perhaps as a result, a great amount of space devoted to the centre and its work appears to

de late that a primary motive for publication was to advertise the centre. Indeed a description of the centre immediately after the following words: "and more forward" by Sir Frank McCadden, the first president and then chairman of the centre (and Shell), followed by a preface by director, Hugh Corbett. In 1970 we learn that Sir Frank is also chairman of the centre's steering group on the reform of the international trading system. Its work was, so it seems, overtaken by the oil crisis of 1973 and again it was obliged to draw attention to the problem. This it did by bringing at the Villa Serbelloni in Lugano on Lake Como at the invitation of the Rockefeller Foundation.

where it produced a Bulletin Memorandum in April 1974, the content of which is reported in this 1976 book.

Thus we are finally brought to focus with the subject of books only after no less than this one pages of "public relations" for an organization established to promote independent analysis and public discussion of communist and other international communist policy issues. As for the book for a slim volume containing no less than such bays: previously appeared elsewhere, *The Economic Crisis in the USSR* is not exactly best buy from amongst the numerous books now available on the crisis.

bad story in Sir Frank McEvedy's foreword, which turns out to be anti-government polemic. Had governments only listened to the national companies, Sir Frank argues, there would have been no wars, no crises, and now what governments have to learn is how to stop pursuing national interests so "broader international" considerations

ations" (of the kind, presumably, pursued by "multinationals") and the "warmer" and "harder" surprisingly, she foravely does not pay my attention to the role of the international oil companies themselves in "exploiting" the "strategic" situation, considering a return to what is considered to be adequate levels of profitability after a fifteen-year period of abnormal "lucrose conditions" in the industry.

This essential element in the understanding of what was happening in the organization of oil in the five years before 1973 (in the period, that is, in the early 1970s) is the oil companies' secret "collusion" on the supply and sale of oil in the London-OPEC (Organization of Petroleum Exporting Countries) area, also missing in the "chapters" of the book.

The "Historical Background to the World Energy Crisis" (by T. Rybczynski and George P. R. Fisher) in the book can hardly be considered a "background" to the "chapters" on the quadrupling of oil prices and the establishment

Thereafter, however, we arrive at the main strength of the book: lies in four chapters which deal with the problems involved in the establishment of a new oil cartel and the consequences of its possible price of oil to a level 10 times higher than that in use in the world economy had been used. This is the work of W. J. van Oort, Peter Oppenheimer, Tumlal and Alisdair Maclean with the adjustments required of international and various national levels and capital flows, the authors very originally written in the last years ago (and are considered in terms of the development of the oil crisis in 1973) factors.

they still provide highly relevant discussions on issues such as inflation/dollarization, higher oil prices, the balance of payments problems for rich and poor countries, and the national monetary and adjustment necessities. A large-scale transfer of resources from the oil-producing countries

[illegible]

However, it is worth pointing out that these chapters, with their outdated assumptions and implicit belief that all would be well as the process of adjustment "takes place" at the national and international level, are the flawed major part of the book. The International Monetary System" by Professor Johnson. The essence of chapter five in his presentation of the problems of the oil crisis in the context of other distortions of the Western economy is particularly revealing. It is not so important that fact, in two ways, however, he odds distance to the book which do not give a clear overall picture. In his

tion that the oil crisis is almost impossible. On the other hand, the potential for a new international financial institution is necessary to warn against the assumption that a new institution should be met by reforming existing institutions according to

[illegible]

will draw the major powers into conflict. And, as the author of the fundamental systems argues, to do anything about it—by doing so—risk relatively small and inconsequential to the habits of living. Professor Auerbach predicted that, and will be taken to be true, arising out of the war in the old world. In this apart from most of the contributors to this book on final analysis see very much the same. The economic system will not adjust—almost autonomous—economic change, and necessarily change, and

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